VI. *On the Inconvertibility of Bark into Alburnum. By* Thomas Andrew Knight, *Esq. F. R. S. In a Letter to Sir* Joseph Banks, *K. B. P. R. S.*

Read February 4, 1808.

MY DEAR SIR,

In a letter which I had the honour to address to you in the end of the last year,* I endeavoured to prove that the matter which composes the bark of trees, previously exists in the cells both of their bark and alburnum, in a fluid state, and that this fluid, even when extravasated, is capable of changing into a pulpous and cellular, and ultimately a vascular substance; the direction taken by the vessels being apparently dependent on the course which the descending fluid sap is made to take.† The object of the present Memoir is to prove, that the bark thus formed, always remains in the state of bark, and that no part of it is ever transmuted into alburnum, as many very eminent naturalists have believed.

Having procured, by grafting, several trees of a variety of the apple and crab tree, the woods of which were distinguishable from each other by their colours, I took off, early in the spring, portions of bark of equal length, from branches of equal size, and I transposed these pieces of bark, inclosing a part of the stem of the apple tree with a covering of the bark of the crab tree, which extended quite round it, and applying the bark of the apple tree to the stem of the crab tree in the same manner. Bandages were then applied to keep the transposed bark and the alburnum in contact with each other; and the air was excluded by a plaister composed of bees-wax and turpentine, and with a covering of tempered clay.

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* Phil. Trans. 1807.

† I had observed this circumstance in many successive seasons; but I was not by any means prepared to believe that such an arrangement could take place in the coagulum afforded by an extravasated fluid; and I am indebted to Mr. Carlisle for having pointed out to me many circumstances in the motion and powers of the blood of animals, which induced me to give credit to the accuracy of my observations; and to that Gentleman and to Mr. Home, I have also subsequently to acknowledge many obligations.

The interior surface of the bark of the crab tree presented numerous sinuosities, which corresponded with similar inequalities on the surface of the alburnum, occasioned by the former existence of many lateral branches. The interior surface of the bark of the apple tree, as well as the external surface of the alburnum, was, on the contrary, perfectly smooth and even. A vital union soon took place between the transposed pieces of bark, and the alburnum and bark of the trees to which they were applied; and in the autumn it appeared evident, that a layer of alburnum had been, in every instance, formed beneath the transposed pieces of bark, which were then taken off.

Examining the organization of the alburnum, which had been generated beneath the transposed pieces of bark of the crab tree, and which had formed a perfect union with the alburnum of the apple tree, I could not discover any traces of the sinuosities I had noticed; nor was the uneven surface of the alburnum of the crab tree more changed by the smooth transposed bark of the apple tree. The newly generated alburnum, beneath the transposed bark, appeared perfectly similar to that of other parts of the stock, and the direction of the fibres and vessels did not in any degree correspond with those of the transposed bark.*

Repeating this experiment, I scraped off the external surface of the alburnum in several spaces, about three lines in diameter, and in these spaces no union took place between the transposed bark and the alburnum of the stock, nor was there any alburnum deposited in the abraded spaces; but the newly generated cortical and alburnous layers took a circular, and rather elliptical, course round those spaces, and appeared to have been generated by a descending fluid, which had divided into two currents when it came into contact with the spaces from which the surface had been scraped off, and to have united again immediately beneath them.

In each of these experiments, a new cortical and alburnous layer was evidently generated; and apparently by the same means that similar substances were generated beneath a plaister composed of bees-wax and turpentine, in former experiments;† and the only obvious difference in the result appears to be, that the transposed and newly generated bark formed a vital union with each other: and it is sufficiently evident, that if bark of any kind was converted into alburnum, it must have been that newly generated. For it can scarcely be supposed, that the bark of a crab tree was transmuted into the alburnum of an apple tree, or that the sinuosities of the bark of the crab tree could have been obliterated, had such transmutation taken place. There is not, however, any thing in the preceding cases, calculated to prove that the newly generated bark was not converted into alburnum; and the elaborate experiments of DUHAMEL sufficiently evince the difficulty of producing any decisive evidence in this case; nevertheless I trust that I shall be able to adduce such facts as, in the aggregate, will be found nearly conclusive.

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* DUHAMEL having taken off, and immediately replaced, similar pieces of the bark of young elms, subsequently found that the alburnum, which was generated beneath such pieces of bark, had not formed any union with the alburnum of the tree, beneath it. But this great naturalist did not employ ligatures of sufficient power to bring the bark and alburnum into close contact, or the result would have been different.

† Phil. Trans. for 1807.

Examining almost every day, during the spring and summer, the progressive formation of alburnum in the young shoots of an oak coppice, which had been felled two years preceding, I was wholly unable to discover any thing like the transmutation of bark into alburnum. The commencement of the alburnous layers in the oak (*quercus robur*) is distinguished by a circular row of very large tubes. These tubes are of course generated in the spring; and during their formation, I found the substance through which they passed to be soft and apparently gelatinous, and much less tenacious and consistent than the substance of the bark itself; and, therefore, if the matter which gave existence to the alburnum previously composed the bark, it must have been, during its change of character, nearly in a state of solution; but it is the transmutation of one organized substance into the other, and not the identity only of the matter of both, for which the disciples of MALPIGHI contend; and if the fibres and vessels of the bark really became those of the alburnum, a very great degree of similarity ought to be found in the organization of those substances. No such similarity, however, exists; and not any thing at all corresponding with the circular row of large tubes in the alburnum of the oak is discoverable in the bark of that tree. These tubes are also generated within the interior surface of the bark, which is well defined; and during their formation the vessels of the bark are distinctly visible, as different organs; and had the one been transmuted into the other, their progressive changes could not, I think, possibly have escaped my observation: nor does the organization of the bark in other instances, in any degree indicate the character of the wood that is generated beneath it: the bark of the wych elm *(ulmus montana)* is extremely tough and fibrous; and it is often taken from branches of six or eight years old, to be used instead of cords; that of the ash *(fraxinus excelsior)* on the contrary, when taken from branches of the same age, breaks almost as readily in any one direction as in another, and scarcely presents a fibrous texture; yet the alburnum of these trees is not very dissimilar, and the one is often substituted for the other in the construction of agricultural instruments.

Mirbel has endeavoured to account for the dissimilar organization of the bark, and of the wood into which he conceives it to be converted, by supposing that the cellular substance of the bark is always springing from the alburnum, whilst the tree is growing, and that it carries with it part of the tubular substance *(tissu tubulaire)* of the liber, or interior bark. These parts of the interior bark, which are thus removed from contact with the alburnum, he conceives to constitute the external bark or cortex, whilst the interior part of the liber progressively changes into alburnum.

But if this theory (which I believe I have accurately stated, though I am not quite certain that I fully comprehend its author*) were well founded, the texture of the alburnum must surely be much more intricate and interwoven than it is, and its tubes would lie less accurately parallel with each other than they do: and were the fibrous substance of the bark progressively changing into alburnum, the bark must of necessity be firmly attached to the alburnum during the spring and summer by the continuity, and indeed identity of the vessels and fibres of both these substances. This, however, is not in any degree the case, and the bark is in those seasons very easily separated from the alburnum; to which it appears to be attached by a substance that is apparently rather gelatinous than fibrous or vascular: and the obvious fact, that the adhesion of the cortical vessels and fibres to each other is much more strong than the adhesion of the bark to the alburnum, affords another circumstance almost as inconsistent with the theory of MALPIGHI, as with that of MIRBEL.

Many of the experiments of DUHAMEL are, however, apparently favourable to the theory of MALPIGHI, respecting the conversion of bark into alburnum; and MIRBEL has cited two, which he appears to think conclusive.† In the first of these, DUHAMEL shews that pieces of silver wire, inserted in the bark of trees, were subsequently found in their alburnum; but DUHAMEL himself has shewn, with his usual acuteness and candour, that the evidence afforded by this experiment is extremely defective; and he declares himself to be uncertain that the pieces of wire did not, at their first insertion, pass between the bark and the alburnum; in which case they would necessarily have been covered by every successive layer of alburnum, without any transmutation of bark into that substance.*

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* Chap. III. Article 5, *Traité d'Anatomie et de Physiologie Vegétale.*

† Chap. III. Article 5.

In the second experiment cited by MIRBEL, DUHAMEL has shewn that when a bud of the peach tree, with a piece of bark attached to it, is inserted in a plum stock, a layer of wood perfectly similar to that of the peach tree will be found, in the succeeding winter, beneath the inserted bark. The statement of DUHAMEL is perfectly correct; but the experiment does not by any means prove the conversion of bark into wood; for if it be difficult to conceive (as he remarks) that an inserted piece of bark can deposit a layer of alburnum, it is at least as difficult to conceive how the same piece of bark can be converted into a layer of alburnum of more than twice its own thickness (and the thickness of the alburnum deposited frequently exceeds that of the bark in this proportion), without any perceptible diminution of its own proper substance. The probable operation of the inserted bud, which is a well organized plant, at the period when it becomes capable of being transposed with success, appears also, in this case, to have been overlooked, for I found that when I destroyed the buds in the succeeding winter, and left the bark which belonged to them uninjured, this bark no longer possessed any power to generate alburnum. It nevertheless continued to live, though perfectly inactive, till it became covered by the successive alburnous layers of the stock; and it was found many years afterwards inclosed in the wood. It was, however, still bark, though dry and lifeless, and did not appear to have made any progress towards conversion into wood.

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* *Physique des Arbres*, Lib. IV. Ch. III.

In the course of very numerous experiments, which were made to ascertain the manner in which vessels are formed in the reproduced bark,[*] many circumstances came under my observation which I could adduce in support of my opinion, that bark is never transmuted into alburnum; but I do not think it necessary to trouble you with an account of them; for though much deference is certainly due to the opinions of those naturalists who have adopted the opposite theory, and to the doubts of Duhamel, I am not acquainted with a single experiment which warrants the conclusions they have drawn; and I think that were bark really transmuted into alburnum, its progressive changes could only have escaped the eyes of prejudiced, or inattentive observers. In the course of the ensuing spring, I hope to address to you some observations respecting the manner in which the alburnum is generated.

I am, my dear Sir,

your most obliged obedient servant,

Elton, Dec. 29, 1807.

THOMAS AND. KNIGHT.

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[*] Phil. Trans. for 1807.